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AUTHOR Cooper, Douglas W.
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ABSTRACT

Today many signs point to an uneasy concern among scholars regarding the ethics of their peers. All too frequently the press reports cases of ethical breaches in many fields of scholarship. Over the years a few people have continued to note and study patterns of deviant and unethical behavior in scholarship. But in spite of the need to discriminate between the various cases of misconduct, there are still those who consider all such misconduct as "fraud." Approximately 30 examples of clearly unethical scholarship, ranging from minor to seriously culpable, are illustrated and examined. They include a variety of cases of irresponsibility, negligence, and fraud, with some practices, such as repetitive publishing and honorary authorship, deserving special mention. A number of causes, including publication inflation and careerism, appear to encourage these breaches with varying consequences. Contains 131 reference notes. (KM)

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A B S T R A C T

UNETHICAL SCHOLARSHIP TODAY: A PRELIMINARY TYPOLOGY

Douglas W. Cooper, Ph.D.
Randolph-Macon College
Ashland, VA 23005

Today many signs point to an uneasy concern among scholars with the ethics of their peers. All too frequently the press reports cases of ethical breaches in many fields of scholarship. Over the years a few people have continued to note and study patterns of deviant and unethical behavior in scholarship. But in spite of the need to discriminate between the various cases of misconduct there are still those who lump all such misconduct together as "fraud." Some thirty examples of clearly unethical scholarship are illustrated and examined here. They range on a continuum from minor to seriously culpable in a variety of cases of irresponsibility, negligence, and fraud with some practices like repetitive publishing and honorary authorship deserving special mention. A number of causes such as publication inflation and careerism appear to encourage these breaches with varying consequences.

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UNETHICAL SCHOLARSHIP TODAY: A PRELIMINARY TYPOLOGY

by
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Introduction

Many signs point to an uneasy concern among scholars with the ethics of their peers. Institutional panels have investigated a host of fraud cases[1] while institutions themselves have begun to draft detailed policies and guidelines for dealing with allegations of misconduct.[2,3] The National Institutes of Health (NIH) are drawing up their own federal guidelines[4] as one of the results of Congressional investigations into fraud in biomedical science begun in 1981.[5] Index publishers add more and more articles and categories on fraud and professional ethics to their lists[6] while some journal editors have even begun scrutinizing manuscripts for possible dishonesty and have talked of how to insure that indexing services print references to retractions along side index entries.[7]

In the last ten to fifteen years there has been a virtual explosion of interest and scholarship in applied ethics. In that time half-a-dozen new associations and research centers[8] and as many new journals[9] concerned with applied ethics have emerged in the U.S. Today associations in anthropology, geography, history, political science, psychology, and sociology as well as the

American Association of University Professors are struggling to formulate new professional codes of ethics.[10] But concern with the ethics of research and scholarship is not new.

Starting more than a century ago a handful of scholars have continued to study patterns of deviant and unethical behavior in scholarship. In 1830 the British Charles Babbage set out several ways researchers tamper with scientific data.[11] Robert K. Merton, a twentieth century American sociologist, studied priority and misconduct among scientists.[12] More recently Harriet Zuckerman followed with her analysis of deviant behavior among scientists.[13] William Broad and Nicholas Wade,[14] Alexander Kohn,[15] and Daryl E. Chubin[16] have written on various forms of misconduct in science. Alexander Lindey[17] and Peter Shaw[18] have examined plagiarism in the humanities, Robert Rosenthal[19] and Theodore X. Barber[20] have looked at error, deceit, and self-deceit in the behavioral sciences, and Paul Davidson Reynolds has surveyed the codes of professional ethics among social scientists.[21] Bernard Barber has examined the role of trust in science,[22] and Nicholas H. Steneck has studied scholarship ethics at the university.[23]

A survey of their writings shows the need for discriminating between the various types of unethical scholarship we find today. For the spectrum of such scholarship includes a great diversity of acts, ranging from conscious neglect and irresponsibility to plagiarism and fraud. The survey has shown a tendency among some writers to lump a variety of these unethical practices together as

"fraud" with no consensus on its meaning.[24] It has also shown that there has been no attempt to inventory the variety of infractions, distinguishing one type from another. Perhaps in the interests of better understanding unethical scholarship in all fields we should take a closer look at some of the many cases and try to see more clearly their characteristics and implications.

A **Scholar** has been variously defined as one who by long systematic study, as in a university, has gained a high degree of mastery in one or more of the academic disciplines[25] and, it might be added, one who occasionally publishes the results of his research. Humanists, social scientists, and scientists all acknowledge a number of essential elements in the professional standards by which they conduct their scholarship.[26] It is to this mixed population and its composite of associated ethics and norms that the present study refers.

What follows, then, is a discussion of a number of ethical breaches forming a preliminary typology of unethical scholarship. Selected instances of misconduct occurring chiefly in the U.S. since 1980 are here drawn from a survey of the literature, both popular and scholarly, and fall more or less logically into various categories. These categories range on a continuum from less serious to more seriously culpable practices to outright fraud. Some of them are more prevalent in science and the social sciences but may be found in the humanities with minor changes in definition. (See Table 1.)

Types

IRRESPONSIBILITY

Several kinds of irresponsible scholarship pose problems of accountability with differing degrees of ethical seriousness. One kind, **false authorship** or the claiming of credit incorrectly, can show up as honorary authorship or as appropriation of a subordinate's work.

The first, honorary authorship, can mislead the reader in assigning responsibility. Honorary authors are those who put their names to papers because they are nominally in charge of the research or simply because they are owed a favor. Increasingly scientists work in teams reflecting a system of growing specialization of expertise and equipment. Often the team leader or principal investigator is not directly involved in the research. Multiple authorship often reflects a complex of reciprocal obligations. But it can sometimes enable a junior scholar or scientist to gain automatic acceptance of his work by associating it with the name of an established or famous scholar. Walter W. Stewart and Ned Feder, writing about Harvard's Darsee case, say that this can lend "a prestige that may lull other coauthors, the reviewers, or the readers into uncritical and inappropriate acceptance."[27] This is a variant of the "Matthew effect" by which notice and reputation accrue to a name already known.[28]

The trend in honorary authorship is on the increase. The Philadelphia-based Institute for Scientific Information has studied the citations in the 2,800 journals that it indexes, including those in the social sciences, and found the average number of authors per paper rose from 1.76 to 2.58 between 1960 and 1980[29] Often the names of a dozen or more coauthors will appear on a paper. One paper in Physics Letters from the European Council for Nuclear Research in 1983 was signed by 138 authors from sixteen institutions.[30] Conversely, the director of William Summerlin's laboratory at Sloan-Kettering Memorial Hospital, Robert A. Good, allowed his name to appear as co-author on 341 papers in the five years preceding the discovery of Summerlin's "patchwork mouse" fraud in 1971, and the practice has also been common at Harvard and Yale.[31]

Several problems attend the practice. Stewart and Feder have found that papers naming at least one such honorary author had significantly more errors than other. The practice also falsifies the true responsibility for published research. Moreover, as their study points out, such honorary authors often fail to use their unique knowledge which makes possible that they spot certain serious errors in published work, errors not discoverable by anyone else.[32] Even more serious, unscrupulous minor researchers can foist deceptive results onto the entire team, sabotaging its findings and opening it to claims of plagiarism or fraud. This seems to have been the case with Vijay K. Soman and Alok

Bandyopadhyay (see below).

The problem of accountability is complicated when team research involves a great many subordinate workers. In 1966 Julius A. Roth studied what he called the "hired hand" syndrome, collecting a series of testimonials from experimenters who admitted that they had fudged their data. Common among such workers is the feeling that they had no stake in the intellectual or professional rewards of the research. Their names are omitted from the published papers.[33]

In another form of false authorship unethical researchers can claim sole authorship in actually collaborative research. The classic case of this occurred when Anthony Hewish wrested the discovery of pulsars from a young woman graduate student named Jocelyn Bell. The Nobel prize in physics went to Hewish in 1974, but in March 1975 the London Times printed a letter from one who was familiar with the discovery, another astronomer, Fred Hoyle, who called the award a "scandal."[34] A variant case in China occurred in the seventies when a superior coerced a subordinate, Ding Zhongqi, to list him as a co-author of his book on the principals and designs of transistor radios.[35]

Irresponsibility of a different kind occurs when a scholar engages in **improper publication**, misleading and manipulating publishers in order to promote his own record of research. Such improper publishing borders on deceit when it takes the form of dual submission, republication, and repetitive publication.

Dual submission or publication can occur when an impatient author, especially in the fast-moving sciences, sends his paper to two or more journals simultaneously. Although the author may intend to retract the manuscript from consideration by others once it has been accepted by one of them, sometimes he is too late. This was the case with Mark W. J. Ferguson in 1981. His article was published in volume 214 of Science and volume 37 of Experientia. His apology appeared in the June 1982 issue of Science.[36] Even without dual publication, simultaneous submission deceives journal editors who in most cases presume they have first rights to the manuscript and a number of referees who expend time and effort to review it.

Equally improper is the practice called repetitive publication which can take several forms. One form is the playing of a kind of auctorial round robin in order to overcome the disadvantages of publication credit practices in some journals. These advantages can be due to "alphabetic disorder" whereby one's name is consigned alphabetically to a less prominent position in a list of collaborators, or from having one's authorship virtually lost as "et al." in subsequent citation. In 1970 two psychologists, R. Over and S. Smallman, found evidence that researchers whose surnames began with the letters P-Z tended to avoid publication in one journal due to its policy of arranging authors' names alphabetically.[37] Thus, if such authors feel that this detracts from the credit they deserve as active scholars in

their field, they may take turns in placing their name first in publishing a slightly modified version of the same findings.[38]

Another form of repetitive publication has been called "salami slicing." In the competitive effort to turn out a large number of publications some scholars commonly slice up their findings into the smallest pieces possible, the least publishable unit (LPU).[39] By this fragmentation trick a single paper can sometimes provide enough material for several minor publications. The practice could be said to cheat the scholarly community, deceive the publisher, trivialize important scholarly communicatio.., discourage full understanding, and withhold information about which the author may be uniquely aware.

Moreover, fragmentation of this kind in science affords an opportunity for dishonesty. The Harvard committee investigating Darsee [40] felt that was so "because such reports are less likely to be verified by others," and, as another writer on the Darsee case observed, because they seem too trivial, because there is "little money and no glory" in replication studies, and because "fudged data that conform to prevailing scientific wisdom can easily slip into print."[41]

Yet another kind of repetitive publication is actually republication and occurs most often in the duplicate or even triplicate publication of scientific abstracts. As Stewart and Feder explain, a scientist may submit an abstract to one meeting of a scientific society with the results of a test so close to

those already published--or submitted to another meeting of the same society--that there is vanishingly little difference between them.[42]

Journal editors and index compilers are concerned about such repetitive publication. Three science editors--Edward Huth of Annals of Internal Medicine, Stephen Lock of the British Medical Journal, and Benjamin Levin of Cells--have condemned it.[43] Over the years several medical journals have evolved rules to stem the tide of repetitive publication and have made plans to notify Index Medicus about violations.[44]

Perhaps as damaging to scholarship as such improper publishing is the problem of **breach of trust.** Delicate threads of trust extend between the scholar and the community of other scholars, his subjects, his hosts, and granting agencies. According to some experts, one way a scholar can break faith with the scholarly community is by excessive secrecy. A recurrent idea in many social science codes of ethics is the obligation each researcher has to share insights and findings with the scholarly community. In 1975 Paul Davidson Reynolds collated a number of codes of ethics from the social sciences and found that these codes expressed the idea in various ways: all research reports should be public documents freely available to all; research with scientific merit should always be submitted for publication and not withheld from public presentation; do not become involved in secret research.[45]

Perhaps secrecy in science is a special case, as Marcel C. La Follette observes. As editor of Science, Technology, & Human Values he feels that secrecy involves a complex web of social and political issues linked to economic motives, national security concerns, and educational ideals.[46] But Bernard Barber speaks for many others when he says that the secretive scientist withholds findings and techniques necessary for the enlargement of the common body of knowledge[47] and thus ultimately hampers the development of social benefits.

However, this principle has run into conflict with the drafting of new codes by several scholarly bodies. Both the American Psychological Association[48] and the American Anthropological Association[49] have agonized over the issue, despite urgings by the National Research Council for scientists to share basic information more openly.[50] Science's norms of openness, "communalism," and disinterestedness[51] tend to waiver under increasingly commercial pressures when both social scientists and laboratory scientists become involved in industry and when the commercial sector demands more and more "university created knowledge."[52]

A stronger case can be made for the injunction against breaking faith by violating confidentiality. One way this is sometimes done is by taking or releasing ideas sent to a grant-making organization or data in a manuscript sent to a journal for review. This was the case at Yale involving Vijay K. Soman in

1978-80.[53] It may also have been the case with some of the 300 cancer researchers recently surveyed, twenty percent of whom believed their data submitted in proposals to NIH had been pirated by reviewers.[54]

A different kind of breach of trust can occur when a researcher uses his research activity as a cover for personal, commercial, or political purposes. For example, according to Robin W. Winks, an historian at Yale, many spies were recruited there from among faculty ranks during World War II and after. But serving as agents for the CIA or its predecessor, the Office of Strategic Services, would, in the words of a colleague, be "utterly inappropriate ... for any scholar of whatever discipline."[55]

This principle has put pressure on scholars who are said to be involved with government agencies while doing research and participating in scholarly conferences.[56] A recent case of this kind occurred when a Harvard professor and director of Harvard University's Center for Middle Eastern Studies, Nadav Safran, revealed in 1985 that he had maintained a "private, confidential" $107,430 research contract with the CIA. When colleagues and participants in a Harvard Conference on the Middle East held in October that year learned later that the CIA helped finance the conference, they asked Safran to resign, charging that such participation may have compromised their standing as objective scholars among their Middle Eastern peers.[57]

Trust and a participant's right to self-determination are at the heart of the concept of informed consent in both social science and medical research. The writings on informed consent are numerous and the question is a tangle of issues about deception, legal rights, and harm versus benefit.[58] At issue are questions regarding research with children and psychiatric patients, people less free to choose, and participants either uniformed or misinformed.[59] Perhaps the Tuskegee scandal demonstrated the most egregious violation of trust in recent memory. From 1932 to 1972 researchers in the U.S. Public Health Service conducted an experiment to monitor the progress of untreated syphilis among 400 blacks in Tuskegee, Alabama, most of whom thought they were being helped by the tests they wre given.[60]

Since then both the U.S. Department of Health and Human Services and the American Psychological Association enunciated principles in a code of ethical standards about informed consent in research.[61] However, John G. Adair, Terrance W. Dushenko, and R. C. L. Lindsay in a 1985 survey of published research found that restrictive ethical regulations have not diminished deception in research with human subjects.[62] Regardless, since 1986 all scientists in the U.S. using human subjects must have their research plans approved by institutional review boards as a condition for NIH funding.[63]

Unethical investigators can violate the trust of hosts as well as subjects. Work done on college or university campuses, in

various institutions or municipalities, or even in friendly nations carries with it certain basic understandings about the terms and limits of that work.

In some politically active and ethically sensitive university communities such as Cambridge, Massachusetts or Stanford, California researchers have repeatedly strained or violated the community trust. When recombinant DNA research was planned in Cambridge in 1976 the community formed a review board and made recommendations to those proposing the experiment that additional safeguards be taken.[64] Then, in October and November 1980, Harvard announced plans to found a potentially lucrative genetic engineering company with research work done by several of its faculty members in its laboratories. It backed down, however, when faculty objected to how it would conflict with the university's traditional academic role.[65] The community itself went to court in March 1984 to block the testing of deadly nerve gas at Defense Department laboratories within the city limits, but lost its case and presumably its trust in the government scientists who went ahead with the tests.[66] In Stanford it was 295 scientists themselves--physicists and associates--who in 1983 presented a petition objecting to a proposal for weapons-related research at their radiation laboratory.[67]

One case of questionable conduct by an American involves working in a host country, China, and raises the question of trust. Steven M. Mosher was a doctoral student thrown out of

Stanford University's anthropology doctoral program in February 1983. It was alleged that he was guilty of "illegal and seriously unethical conduct" while conducting field research in a mainland Chinese village. He was thought to have been involved there in bribery or smuggling in order to obtain secret local government documents. When the host government discovered his activities and refused to extend his visa, the now-expelled and angry Mosher published in Taiwan an article criticizing mainland China's birth control policy and the problems with it which he had witnessed. The article included photographs revealing the identity of villages whose trust he had previously held.

By such action Mosher appeared to show little sensitivity and fewer ethical scruples. He flouted the anthropologist's code of professional ethics, one item of which requires researchers to protect the welfare and confidentiality of those observed. By his actions he not only violated the trust of a host nation but endangered the joint American-Chinese arrangements for long-term field research in China.[68]

Slander and recrimination play important parts in the history of unethical scholarship and demonstrate the breach of unwritten codes of scholarly conduct. Publicly impugning a person's reputation or work by ill-founded charges, particularly if done as recrimination against an accusor, has been quite common. One scholar has earlier traced the history of slander in science where priority of discovery engenders among some people excessive

competition, envy, slander, and recrimination, sometimes even leading to "outright deceit in order to buttress valid claims."[69]

At times those who seek to expose misconduct are themselves subject to counter-accusations and harassment. A political scientist at Columbia University, Alan F. Westin, has pointed out how, in numerous recent cases of plagiarism and fraud, the perpetrators have suffered less than those who exposed them.[70] Two such cases involved scientists Jerome Jacobstein at Cornell in 1981 and Bruce Hollis at Case Western Reserve University in 1982.[71] In another example, in 1983 a well-established researcher, Robert Sprague, suffered withdrawal of a national grant when he pressed the National Institute of Mental Health (NIMH) to investigate the fabrications of a leading University of Pittsburgh psychologist, Stephen Breuing, whose research had a direct impact on health policies.[72] Fortunately Sprague was vindicated in 1987 when a NIMH panel found Breuing guilty of "serious scientific misconduct," barred him from receiving grant funds, and referred its findings to the Department of Justice for possible action.[73]

While outright **breaches of law** such as theft or destruction of property, equipment, or products is not unknown in the world of research, simple disregard for the law and rules shows an irresponsible attitude toward scholarship, as well. However, experimentation is not always easy to accommodate to the rules in rapidly changing fields of science. In 1981 a medical researcher

contravened federal regulations by his use of recombinant-DNA molecules in treating several of his patients. Martin J. Cline who was chief of UCLA's hematology and oncology division resigned after investigations began, he was censured by the NIH, and more than $250,000 in NIH grant funds were reconsidered. He had disregarded both federal regulations for the protection of human subjects and NIH guidelines for the use of recombinant-DNA technology. He had also violated the U.S. Department of Health and Human Services' research requirements.[74] Finally he was stripped of two of four grants with the withdrawal of funds totaling $190,000.[75] Four years later the NIH hammered out guidelines for human gene therapy experiments,[76] guidelines which it continues to revise as knowledge in the field develops.

NEGLIGENCE

The writer who knowingly allows the **presence of serious errors** has shown a lapse from the standards of veracity and accuracy which are the trademarks of scholarship and can cast doubt on his own integrity. Careless note-taking can undermine his finished manuscript. Incorrect documentation can mar his otherwise well-written article. Faulty research design or conduct can impune the reliability of his experiment.

The degree to which the presence of errors is, by the standards of the time, unacceptably sloppy work, but not fraudulent work, determines whether it enters the realm of what

Harriet Zuckerman has referred to as "disreputable error."[77] Such error violates standards of acceptable scholarly practice, calls derision and contempt on their perpetrators, brands them poor scholars, and damages their reputations.

Several studies have examined the many types of errors to which the behavioral sciences are prone. In an extensive 1966 analysis Robert Rosenthal discussed many types of errors. Among others, he discusses errors of observation, recording errors, errors of computation, and errors of interpretation,[78] all of which plague the work of researchers in most disciplines, not just the behavioral sciences. Other writers like Theodore X. Barber have examined similar errors.[79]

Reputable errors occur sometimes and to all scholars despite their efforts to maintain high standards in technical norms and procedures. Such errors are expected as occasional slips but they do not constitute a breach of ethics. Unfortunately they can create problems long after their occurrence. For example, John R. Sabine reported that two papers first published in 1980 were retracted in July 1983, by which time they had already been cited by other articles thirty-nine and thirty-one times, respectively.[80] Some journals are even reluctant to publish retractions for clearly fraudulent papers.[81]

However, the extent to which those conducting and reporting such research are guilty of disreputable error, that is, unethical acts, depends on whether it displays gross negligence. It also

depends on the readiness of its authors--if given the opportunity-to retract, correct, admit, or point out these errors once they are know. Several recent studies of biological papers, one by G. de Lacey, C. Record, and J. Wade and another by John R. Sabine, show that it is a rare author, some two percent in one study, who will acknowledge such errors in print.[82]

Incorrect documentation constitutes a different lapse from proper scholarly standards. Omitted citations can be interpreted as plagiarism, and pageless[83] or inaccurate citations can pose serious impediments to other researchers following. De Lacey, Record, and Wade's study in 1985 looked at one month's articles in six leading science journals that year and asked the question "How accurate are quotations and references?" They had to answer ruefully that the original author was misleadingly misquoted in 15 percent of all references, and errors in citation occurred in 24 percent, of which 8 percent were major errors preventing immediate identification of the source.[84] It is doubtful whether publications in the humanities and social sciences have a much better track record.

One case of omitted credit points up the responsibility borne by journal reviewers and editors, as well. When Isaac Ginsburg saw that a paper in Infection and Immunity gave no credit to him for the same findings he had published during the past two decades, he wrote the editor. The editor recommended that the two authors offer a private apology, since he felt it was merely an

error of omission without malice or dishonesty. One of the two authors was a member of the editorial board and bore part of the responsibility for the unconscious plagiarism.[85] Finally, though, the editor's task of "100 percent detection" of such plagiarism is "probably impossible," according to Marcel C. La Follette.[86]

Detecting the use of "secondary reference" is probably equally impossible although of less gravity. Some critics feel quite strongly about it. As Mary-Claire VanLeunen explains, "second-hand information must be presented as second-hand. To treat it as if it came from the horse's mouth is dishonest."[87] Treating a secondary reference as if it were primary is not only dishonest but dangerous. According to Jacques Barzun and Henry F. Graff, experienced journal editors say that some 15 percent of all footnotes in the average article contain errors of various kinds.[88] Ultimately, if the use of secondary reference were a safe and acceptable practice, what would prevent a researcher from gathering his supporting evidence directly from lists of citations in indexes, abstracts, and bibliographies? One might wonder how many "scholars" already are guilty of this shortcut.

The problem of **self deception** is a grey area lying somewhere between carelessness and deception combining elements of both. For this reason ethical culpability is more problematic. It pervades all investigation and often confounds the most otherwise exacting efforts at observation and discovery. Inasmuch as self deception results from a scholar's failure to treat his own work

with as much skepticism as he treats the work of others, it is a form of negligence. Harriet Zuckerman argues that it violates both the social and cognitive or technical norms of disinterest and skepticism expected of researchers.[89]

Self deception can cause the unconscious formation of results by numerous subtle means. One means is the expectancy effect, or the self-fulfilling prophecy.[90] Another is the digit preference effect. By this the cumulative result of choosing to notice or record a digit higher or lower--even by only one point--can be the difference between black and white.[91] While expectancy leads to self deception, "trimming" or prettifying can verge on deception. Trimming, or a preference for data that appears to be more symmetrical, consistent, or accurate comes about when, in the words of Charles Babbage, the researcher clips "off little bits here and there from those observations which differed most in excess from the mean" and sticks them "on to those which are too small."[92] Numerical trimming is only one type; another is theoretical or notional trimming which can lead to the "fudging" of ideas.

DECEPTION

Data Manipulation presents a somewhat more serious moral problem than trimming. One of its forms is selective reporting, sometimes called "cooking" or "fudging" the data. In more general terms this can occur when a writer tends to exaggerate his case by

minimizing exceptions and evidence contrary to his argument. It can take various forms with the intention of having the observations support a favored theoretical position. From among a multitude of observations the researcher selects only those which support his case, suppressing (ignoring and omitting) all others. According to Rosenthal, such intentional suppression of evidence is akin to the fabrication of data.[93]

Sometimes carelessness or misuse of sources can also become deception. One case involves a young historian's questionable use of published sources and has provoked a continuing controversy among history scholars and others. It began in 1983 when Henry Ashby Turner, Jr. of Yale attacked another historian and author of a study on big business in Germany's Weimar Republic. Turner was joined by Gerald D. Feldman, an historian at Berkeley, in charging David Abraham, formerly an assistant professor at Princeton, of deliberately misattributing, misquoting, and generally falsifying evidence in his book, The Collapse of the Weimer Republic (1981), and of "tendencious misconstrual of the evidence." Abraham admitted mistakes were made but defended himself in part by claiming they were merely from careless note-taking.[94] At what point, one might ask, does the use of misleading quotes in his "careless note-taking" become manipulation of his sources to create an argument otherwise unsupportable?

A recent case from Japan shows how another historian attempted to rewrite the record. Overwhelming evidence indicates

that in 1937, during the invasion of China, the Japanese troops massacred more than 200,000 Chinese, including civilians, in Nanjing. Despite this evidence Masaaki Tanaka, a Japanese scholar, in 1985 with his The Myth of A Nanjing Massacre published the field record of General Iwane Matsui, commander at Nanjing, clearing the Japanese of any wrong-doing. Under close scrutiny, however, a group of Japanese China scholars found that Tanaka had altered the text of the record with more than 900 insertions and deletions. When confronted with the falsification, Tanaka claimed the errors were caused by "eye strain."[95]

Misuse of grants and grant funds is as bad as or worse than misuse of sources. The possibility of **grant fraud** increases in proportion to the overall increase in grants which has boomed in some scientific fields. Dr. Robert Petersdorf, dean of medicine and vice-chancellor for health sciences at the University of California at San Diego, says federal research grants to medical school grew from $436 million in 1960 to $2,351 million in 1981, increasing proportionately much faster than enrollment. But he felt that cheating to obtain grants had eroded the moral fibre of many physicians.[96] The NIH investigates cases of fraud involving federal research grants. Although NIH gets only two fraud complaints each month from among the 20,000 current research projects at its eleven Institutes, the Institutes are not at present required to report cases of suspected fraud.[97]

Yet, various devious strategems continue to surface. The

commonest are described by a professor of biology at the University of Chicago, Leigh Van Valen, editor of Evolutionary Theory and critic of irresponsible publishing. One involves the seeking of support for work already completed but not yet published. Since the results are already known the project can be described knowing it will succeed. This can gain grant support for new research for which the grant is not intended. Another involves proposing to do research which will be done if no new ideas come before the grant ends. Both ploys deviate from the conditions for which the grant is awarded and must therefore be considered dishonest. When the granting agency knows of them it means automatic rejection of the application.[98]

While these practices might be seen as less culpable than some forms of deceit, the line between them and fiduciary irresponsibility is a fine one. Grant applicants commonly inflate expected costs and sometimes take portions of grant funds or equipment for their personal use. But fiduciary responsibility requires accountability. When Darsee's Harvard fraud was exposed, NIH asked Braunwald's laboratory to return some $120,000 of federal grant money spent on the study which had been declared useless because of invalid data.[99]

A form of deception more blatant than grant fraud is the pretense that someone else's words or ideas are your own. **Plagiarism** is a moral offense and, in some cases, may constitute a violation of civil law. At the least the penalties for plagiarism

can include loss of respect among one's peers or even loss of professional status or employment. The American Association of University Professors is only one of the many professional groups that have formal statements on a writer's responsibility to observe intellectual honesty and to acknowledge "academic debts."[100] Part of the professional socialization process involves learning to do this. As Mary-Claire VanLeunen puts it,

> most of us who went to American grade schools can remember long hours of copying articles out of encyclopedias. "The abode of the penguin is a hard and difficult one." It was called doing research. Then in college we found that it was also called plagiarism.[101]

Even self plagiarism is thought wrong because, as one critic observes, it "signifies lack of scientific objectivity and modesty," "reflects sterility and constitutes exploitation of what is considered an ethical medium for propagandizing one's self."[102]

Society at large appears to vascilate on how seriously it views plagiarism, unless it is scientific plagiary. Musical[103] and literary plagiarism[104] have flourished from the Renaissance to modern times. Many journalists today regard plagiarism as an inevitable fact of life in a business which deals in the telling, adapting, and relaying of wire stories.[105] But science is a different matter.

In the late seventies and early eighties scholarly plagiary and fraud made the news in a big way. During the year 1978-80 Dr.

Vijay R. Soman caused a stir at Yale University when he lifted passages as well as laboratory findings from a paper by someone else, a Dr. Helena Wachslicht-Rodbard.[106] The plagiarism was compounded with serious problems of irresponsibility and a reluctance to report the incident, as follows.

First, an abuse of confidentiality occurred when Dr. Philip Felig who directed Soman's research and was at the same time a reviewer for the New England Journal of Medicine received Rodbard's paper for review and passed it to Soman to see, contrary to the journal's practice. After Soman lifted information from it--unbeknownst to his supervisor--, Felig recommended rejection of Rodbard's paper, then only one month later allowed his name to be given as coauthor to Soman's paper sent to another journal, the American Journal of Medicine. As director of the research, coauthor, and reviewer who was fully aware of the similarity between the two papers, Felig's action can easily be suspected of irresponsibility if not connivance in the plagiarism.

After investigations exposed Soman's theft as well as other examples of his falsifying data, Soman resigned and Felig withdrew twelve of fourteen earlier papers done using data supplied by Soman, Felig himself lost a much-coveted post at Columbia where he was being considered for appointment, but he retained his tenured professorship at Yale. Soon after this he testified before a U.S. Congressional probe into biomedical fraud.[107] His rebutal to accusations of complicity, or at least irresponsibility for the

dishonest behavior of his subordinate, was that he had "forty full-time people, including eight nurses and data managers" working on the project and that he was not fully conversant with the methodology Soman used.[108]

A more recent case shows how carelessness in crediting one's sources can blur the distinction between plagiarism and improper research behavior. It involved the chairman of Stanford Medical School's Department of Medicine, professor Kenneth Melmon, who contributed to an edited medical textbook. In the spring of 1984 the university's ethics committee and two publishing concerns began investigating allegations of plagiarism.[109] In June Melmon resigned his chairmanship when he was censured by the ethics committee for "grossly negligent scholarship." Melmon claimed that the text's editor, before his subsequent death, assured him that proper attributions and copyright permissions would be done. But Melmon knowingly submitted the work containing 24 percent plagiarized writing.[110]

Plagiarism can easily become **forgery** when entire articles are pirated. This is what happened with the systematic theft committed in the late seventies by an unscrupulous and ambitious scientist, Elias A. K. Alsabti, who came to the U.S. from Iraq on a fellowship in 1977. While working at research centers in the U.S. between 1978 and 1980 he put his name to some sixty scientific papers published in specialized journals in Japan and the Middle East.[111]

Another case of forgery, this time in the social sciences, made the news in 1984. It seems that two years earlier Conway L. Lackman, a Rutgers University professor published in the Quarterly Journal of Economics an article written by someone else. By the May 1984 issue of the journal the editors republished the article with a notice giving full credit to its true author, Larry Chenault of Miami University.[112] The press noted Lackman's resignation the same month.[113]

Whether it be considered forgery or simply fraud, fabrication has been found in many cases of behavioral and biomedical science deception, including the well-known cases of Summerlin, Straus, Long, Spector, and others.[114] It can involve questionable manipulations as well as outright invention, with a combination of trimming, cooking, or fudging. It can result from "pushing" the data, that is, letting desires and biases influence the way the data are analyzed or reported, or even dishonestly putting together a work or study.

For example, Leon Kamin in 1974 reexamined the papers of one of the most esteemed British psychologists, Sir Cyril Burt, and found that this study of intelligence conducted over several decades was riddled with statistical errors and flagrant inconsistencies and referred to two apparently non-existent co-workers. He showed that Burt had at least cooked the data or possibly even invented it outright in order to demonstrate the inheritance of intelligence.[115] Moreover, he is reported to have

plagiarized the work of others and to have altered articles submitted to the British Journal of Statistical Psychology of which he was editor, sometimes distorting their conclusions so as to fit his own theories.[116]

In another scandal Dr. John Darsee compressed a two-week experiment into a few hours while working at Harvard Medical School. But when his findings contradicted results from other labs this fabrication was discovered. In 1981 he confessed to a single forgery, but colleagues and investigating committees subsequently found that he had fabricated much of the data which were the basis of his more than one-hundred publications over a period of some three years while at Emory and Harvard University medical schools.[117]

Like the plagiarism case at Yale, the responsibility of a principal investigator was at question, as Darsee was working on a multi-institutional study sponsored by NIH under the direction of Dr. Eugene Braunwald. Together they collaborated on five additional articles which were later found to contain "extensive irregularities" according to the NIH-appointed investigating panel.[118]

The case of Michael Briggs and the contraceptive pill is one of the latest involving a major deception of a kind called "dry labbing." Briggs recently admitted that, in the biggest study published in support of the pill, he had not actually performed tests he earlier claimed to have done. Instead he collected from

other researchers small-scale findings from which he fabricated a broad and sophisticated trial that was never conducted. When he quit his post at an Australian university in 1985 the British doctors who had for so long given him a high place in the medical community no longer gave any credibility to the findings of his articles published and cited in numerous learned journals.[119]

The same year that Briggs resigned there emerged in the U.S. another case of faked data compounded by honorary authorship. Investigations began at the medical school of the University of California at San Diego (UCSD) which in 1986 revealed that Robert Slutsky, a junior scientist, fabricated data which invalidated a total of sixty-eight medical papers and implicated dozens of co-authors. He had faked data in three papers, invalidated ten others which used his data, and damaged the validity of fifty-five others. He resigned, withdrew fifteen published papers, but refused to acknowledge fraud. For a time at UCSD he had produced papers at the rate of about one every ten days, altering research results arrived at by younger colleagues, and adding their names and the names of senior researchers as honorary authors. Some welcomed or acquiesced to honorary authorship despite Slutsky's suspicious productivity; others were not even aware he had added their names as authors.[120]

Unlike Harvard's investigation of Darsee, UCSD's dean put all of Slutsky's co-authors on notice that they would have to defend their papers. Despite their objections the papers were classified

"questionable" for twenty-five co-authors and actually "fraudulent" for thirteen young researchers. Seven senior faculty members are now co-authors of questionable papers and three of fraudulent ones.[121]

Howard Eisen's suicide in February 1987 may be seen as flowing from co-authorship of a paper suspected of fraud. According to the report, his wife said he "reviewed" the paper and "was one of the authors." At Eisen's laboratory in NIH's National Institute of Child Health and Human Development a certain Alok Bandyopadhyay collected data for the collaborative study but was dismissed after a review of the paper--which was to be published in the Proceedings of the National Academy of Sciences--revealed inconsistencies in the data. Eisen then performed experiments that convinced him that Bandyopadhyay had adulterated his data. NIH made no allegations against Eisen[122] but presumably he took responsibility for his role as co-author.

Finally, there is great **reluctance to report major misconduct** when known. June Price Tangney of Bryn Mawr College found that nearly one-third of scientists and social scientists surveyed at a major university said they suspected a colleague of fraud but more than half of those said they had done nothing about it. Her survey indicates that "there is a generalized reluctance to take prompt, corrective action." Perhaps this is so because, as innocent-bystander research indicates, the more people who witness a wrongdoing, the less likely each bystander is to

respond.[123]

Nicholas Steneck attributes the reluctance to act to both psychological and bureaucratic impediments.[124] Daryl Chubin has similar views. He suggests that the institution's good name confers a presumption of probity on the researcher who does work there and that this forestalls investigation.[125] For, if misconduct is publicly exposed, heads of laboratories risk their livelihood, laboratories their reputation and their funding, and researchers all of these plus the goodwill of their colleagues. With ethical injunctions set against false accusation and slander, and academic freedom policies inhibiting institutional investigation,[126] the impulse is to presume innocence or keep the problem in-house. To this presumption of innocence is added the inertia of honorary authorship which diffuses accountability and responsibility. Honorary authorship can also make for connivance among collaborators who are reluctant to lose additional publication credit.

Conclusion

Unethical scholarship today comes as always from various causes. There are social, professional, and institutional ones such as careerism, improper professional socialization, the advent of "big science," competition for grants and promotion, pressure to publish, increased numbers of scholars and researchers. There are also personal ones such as amorality, negligence, moral

weakness, or indifference.

Critics explain it in different ways. Steneck writes that sometimes the altruistic interest in advancing knowledge collides with a concurrent personal desire to gain professional and financial rewards, producing in a few scholars the lapses in scholarly integrity which come to light as breaches in ethical practice.[127] Cubin sums up the explanation of many experts in another enumeration: "psychopathy, unbridled ambition, pressure for publication, competition for federal support, the 'lab-chief' system, failings of the 'peer-review system,' lack of replication, and sheer sloppiness."[128]

Sheer numbers in the profession pose a problem for honest scholarship. Over the years, higher education has become an industry turning out more and more new scholars. As careers advance by a swelling bibliography of publications, the number of journals grows. Thus, the eventual ocean of publications in which the bad or fraudulent paper is easy to overlook. The numbers are huge. The largest national indexing association in the U.S. reported in January 1987 the previous year's figures for its fifty-three member and associate member indexing to be 5,939,506 titles in a single year.[129]

As a result of this flood of publications many scholars feel swamped and under pressure. A recent survey of 3,800 scholars conducted by the American Council of Learned Societies (ACLS) found that sixty percent felt it was "virtually impossible to keep

up 'even minimally' with the literature in their fields." The same survey found that more than half of the scholars in research universities felt the pressure to publish was "extremely strong" and a third of those in comprehensive colleges or universities felt it to be "strong."[130] An earlier survey organized by the ACLS found that seventy percent of "senior scholars" felt institutional pressures to publish. If these pressures themselves are not resented, it said, they led scholars to resent the use of a publication record as a criterion of their performance and worth, that is, the assessment of worth by quantity, not quality.[131]

No matter what the causes, the diversity of unethical acts in scholarship means that a variety of effects are produced in the scholarly community and in the public itself. Some are of minor significance, some major, and others problematic but potentially serious:

- Incorrect documentation and disreputable error act as impediments and irritants to careful research both for those producing it and for those who follow. They also detract from a scholar's record of care and accuracy and may cloud the record of his integrity.
- Lapses in responsibility produce additional problems. For example, breaches of trust with one's subjects or hosts jeopardize public trust and impune the credibility of researchers; honorary authorship can allow laxity and

mischief to creep into collaborative work; improper publishing tends to mislead, even deceive the scholarly community about the scholar's true work, trivializes publications, and wastes time and effort of reviewers, publishers, and researchers.

- Deception and fraud in their various forms can produce long-ranging effects on trust within the scholarly community and among the public. Particularly in science and social science, deception can have a powerful impact on human life and social welfare, as well as on individual reputations, institutional credibility, public trust, and public policy.

Many kinds of ethical lapses, including some not examined here, can have serious and unforeseen consequences. Differing classifications of unethical scholarship prevent a clear discussion of instances of misconduct and so-called fraud. This topology thus may be seen as an effort to clarify the nature of some of the types in this class of deviant behavior during the current concern with ethics, research, and scholarship.

Table 1. Preliminary Typology of Unethical Scholarship

I. IRRESPONSIBILITY
- A. False Authorship
  - 1. Honorary Authorship
  - 2. Appropriation of the Work of a Subordinate or Collaborator
- B. Improper Publishing
  - 1. Dual Submission/Publication
  - 2. Repetitive Publication: Abstracts Republication, "Salami Slicing," Auctorial Round Robin
- C. Breach of Trust
  - 1. Of Scholarly Community
    - a. Secrecy/Withholding Information
    - b. Abuse of Confidentiality
    - c. Using Research Activity as a Cover
  - 2. Of Subject(s)/No Informed Consent
  - 3. Of Host Institutions, Municipalities, Nations
- D. Slander and Recrimination
- E. Breach of Law

II. NEGLIGENCE
- A. Disreputable Error: Serious Errors of Observing, Recording. Computing, Interpreting
- B. Incorrect Documentation
  - 1. Omitted/and Plagiarism
  - 2. Inaccurate
  - 3. Misleading (Eg. Secondary Quotation: Citation vs. Acknowledgment)
- C. Self Deception
  - 1. Expectancy Effect
  - 2. Digit Preference Effect
  - 3. Symmetrical Data Preference: "Trimming" of Variance for Consistency

III. DECEPTION/FRAUD
- A. Data Manipulation
  - 1. Selective Reporting: "Fudging" or "Cooking" the Data
  - 2. Misuse of Sources: Tendentious Misconstrual, Misleading Quotation, Misattribution
- B. Grant Fraud
- C. Plagiarism: Plagiary of Passages or Ideas/& Omitted Citation
- D. Forgery
  - 1. Plagiary of the Majority of a Publication
  - 2. Fabrication of Data: "Dry Labbing"
- E. Cover-up/Failure to Report Major Misconduct

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